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11/12/98

Bruce Halstead
US Fish & Wildlife Service
1125 16th Street, Room 209
Arcata, CA 95521

RE: Permit numbers PRT-828950 and 1157

John Munn
California Dept. of Forestry
1416 Ninth Street
Sacramento, CA 95184

RE: SYP 96-002

Dear Messrs. Halstead and Munn,

Please find enclosed my comments on the Pacific Lumber HCP/SYP. As you will note from my comments, I do not consider the proposed plan to adequately meet the criteria for an acceptable HCP. The basis for my conclusion should be clear upon reading my comments. I have also included a copy of my CV so you can assess my ability to assess the plan. Thank you for the opportunity to comment on this important issue.

Sincerely,

Dr. David R. Montgomery
Associate Professor

Comments on the PALCO HCP, Public Review Draft

David R. Montgomery, Associate Professor
Dept. of Geological Sciences, University of Washington.

Summary of Comments

Based on my reading of the PALCO draft HCP, I conclude that the HCP does not comply with the requirements in regard to coho salmon that the HCP will: 1) to the maximum extent practicable, minimize and mitigate the impacts of taking; 2) insure that actions undertaken under the HCP will not result in the destruction or adverse modification of habitat determined to be critical; and 3) use the best available scientific understanding and data. Below I summarize my conclusions regarding the ability of the plan to adequately protect coho salmon in the area of concern for the life of the plan. More detailed comments keyed to particular sections of the plan follow these general comments.

DRM-1

Issue 1: the plan provides no riparian protection for headwater channels. This approach ignores both the importance of sediment storage in headwater channels in attenuating delivery of high sediment loads in mountain channel networks, with the probable result that continued delivery of high sediment loads from upstream will sustain degraded conditions downstream as a result of future actions upstream. Large woody debris can be a significant sediment storage element in headwater channels and its depletion through either direct removal or reduced recruitement can reduce the amount of sediment stored in headwater channels, and thereby delayed from entering downstream portions of the channel network. Sediment delivery to and transport within the channel network strongly influences downstream habitat conditions and in my opinion ignoring the impact of changes in sediment storage in headwater channels on the state of downstream channels fails the test of the criteria enumerated above.

DRM-2

Issue 2: narrow no cut buffers on Class I and Class II streams. The narrow no cut buffers along Class I and Class II streams will allow removal of the largest trees from riparian zones, even though research has documented the importance of the largest trees in riparian zones for providing in-stream functions and in catalzying positive habitat benifits from smaller wood debris that is routed through the channel system. Based on my field experiences working in streams within buffers of various widths in Oregon, Washington, and Alaska, and on previous assessments of buffer widths needed to

retain riparian functions (e.g., FEMAT, 1993), I do not consider 10' and 30' no cut to be adequate for providing long-term maintenance of riparian functions. Moreover, such narrow no cut buffers do not allow for channel movement across the acknowledged channel migration zone. The net result of the proposed actions will be long term depletion of in-channel woody debris, especially in locations where the channel is unconfined and may wander across its floodplain. Again, this aspect of the plan fails to meet the criteria outlined above.

DRM-2 con.

Issue 3: landslide hazard mitigation.

The plan is also plagued by inconsistencies in regard to landslide hazard planning. While there is some mention of bedrock hollows as "extreme" hazard, there appears to be no technical details of how analysis will be done to identify such areas, and the areas shown on the map of landslide hazards presented in Volume V appears to under represent areas of "extreme" hazard in an area in which high uplift rates and deep river incision make the terrain very susceptible to landsliding. In my experience the distinction between very high and extreme hazards is certainly not identifiable with the soils and geologic maps used to identify such areas in the HCP. While the default prescriptions (Volume IV, Section 3, page 14) for application before watershed analyses are completed are conservative, after watershed analyses are completed the plan allows cutting trees and reducing root strength in extreme, very high and high landslide hazard areas. After watershed analysis a review by a geologist is required prior to cutting in "extreme" hazard areas, and before watershed anlaysis a review by a forester and a geologist is required to cut high, very high and extreme landslide hazard sites. Recent experience in Washington suggests that the recommendations arising from such a review may not be followed in the development of site-specific prescriptions following completion of a watershed analysis (Collins and Pess, 1997). Furthermore, there is no technical basis for the assertion that timber cutting (including partial cuts and shelterwood approaches) in high and very high hazard areas will not result in accelerated landsliding -- such a belief rests on the untested, and likely incorrect, idea that the resprouting of redwoods makes root strength a non-issue for slope stability. In my opinion, the proposed landslide hazard mitigation measures are inadequate for meeting the criteria outlined above. Landscape management under this scheme will result in continued management-driven elevation of landsliding at rates well above background, albeit the

DRM-3 con.

rates will likely be less than under previous forest management practices.

Issue 4: habitat assessment procedures.
The habitat assessment procedures proposed in the plan will make everything appear either OK or intermediate in condition through use of metrics that should be expected even in poor-condition habitat or through comparison with general existing conditions in the region, which for the most part represent already degraded conditions. In particular, the use of pool frequency and percent pool area targets that should be met in all but the most extremely degraded habitat fails to meet the criteria given above.

ORM-4

Issue 5: watershed analysis and adaptive management.
The proposed use of watershed analysis has enough caveats placed on the range of allowable prescriptions and the subsequent feedback between monitoring, new scientific understanding, and managment decisions so as to completely undermine the use of the plan to implement adaptive management. And yet some form of adaptive management is central to generating confidence that the actions undertaken during the life of the plan will meet the criteria outlined above.

ORM-5

Based on these considerations, and the comments that follow, I conclude that the proposed HCP is inadequate for meeting the objectives for an HCP outlined under section 10 of the Endangered Species Act as future management actions will allow significant taking of coho and adverse modification of coho habitat as a result of depleted in-channel woody debris, accelerated sediment delivery from headwater channels, and continued elevation of landslide rates significantly above background levels.

ADDITIONAL COMMENTS

Volume 1. Summary
1. Two facets of the proposed use of Watershed Analysis (page 56) merit additional comment.

First, the Washington method is touted as "replicable and scientifically based". With minor caveats I agree, but Washington's method was not designed for use in HCP's. It was designed to meet State Forest Practice Standards in a context lacking the mandate of meeting performance expectations under the Endangered Species

ORM-6

Act. A recent review (Collins and Pess, 1997) of the management prescriptions adopted under the Washington framework demonstrated that many of the prescriptions adopted under the Washington framework had little to do with the scientific basis of the analyses conducted. Hence, any serious effort to "adapt the methodologies to California conditions", as the PALCO document states is necessary, must focus not only on the technical methodologies that the PALCO document appears to be referring to, but also the entire decision-making context within which that information will be put to use. More specifically, an explicit assessment of the level of risk that will be acceptable in shaping the prescriptions will ultimately drive whether the plan succeeds in adequately protecting aquatic organisms. The level of acceptable risk for an HCP should be different than that for regulating state forest practices without such a listing.

DRM-6 CON.

Second, no detail is offered about how the methodolgies need to change in order to reflect "California conditions". Many of the methodologies in the Washington method are general in nature and the assertion that the methodologies need to be adapted for use in California should not be left at such a general level. In order for NMFS to properly evaluate the potential for taking and adverse impacts on coho habitat the specific changes need to be elucidated.

DRM-7

2. The Sediment Assessment section on page 57 raises a number of concerns. Is evaluating 4 basins in 10 years really as good as the company can do to address the acknowledged need to address the important issue of road-derived sediment and associated disturbances? An average time of 2.5 years to analyze each watershed sounds like a pretty slow pace for addressing issues that have gotten to the point that the ESA has been invoked.

DRM-8

3. In the road construction and maintenance section it is implied that a road that is "well drained and shows no signs of imminent failure (as evidenced by slumping scars or cracks in the road fill)" is considered upgraded. While it is obvious that PALCO should without delay address roads known to be in the process of failing (slumps and cracks being evidence of an already initiated, but not fully developed failure), the criteria are clearly inadequate for identifying poorly built or maintained roads that have yet to begin failing, but are at high risk for doing so.

DRM-9

ORM-10

4. The section on Hillslope Management (page 59) leads me to conclude that landslide issues will not be adequately addressed under the plan. The only locations where timber might not be cut due to slope stability concerns are in locations with an "extreme" mass wasting potential. All that is needed to cut even in these areas is "a geologist's report recommending alternative prescriptions that are approved by CDF". There are no demonstrated methods of timber harvesting for sites of "high", "very high" or "extreme" mass wasting hazard that will not increase the potential for slope failure. It is not clear from the HCP what the standards used to judge the "alternative" prescriptions will be. Hence, in my professional opinion I cannot conclude that such prescriptions will prove to be more than uncontrolled experiments, and hence do not meet the standrards of using the best available science to minimize the potential for taking of coho or adversely impacting coho habitat.

In addition, the plan allows logging without site-specific review in areas with a "high" or "very high" rating for mass wasting hazard. Examination of the maps included in the HCP (Map 13. Landslide Hazard Index) shows that little of the area (<1%?) is considered at "extreme" hazard, whereas a much larger area is given a high or very high rating. The best available science indicates that post-harvesting root strength loss in sites with high to very high mass wasting potential will increase the potential for slope failure due to reduction in the soil reinforcement provided by roots (Burroughs and Thomas, 1977; Waldron, 1977; Ziemer and Swanston, 1977; Gray and Megahan, 1981; Waldron and Dakessian, 1981; O'Loughlin and Ziemer, 1982; Burroughs, 1985; Buchanan and Savigny, 1990; Sidle, 1991; Reistenberg, 1994). Hence, the HCP again fails the test of using the best available science to guide its prescriptions.

The third sub-section of this section appears then to contradict the first two by stating that CDF and other agencies will have the ability to "review" THPs in all three stability classes (extreme, very high, and high) and to determine whether mitigation measures will avoid significant impacts. I know of no scientific literature that demonstrates how to harvest trees without the potential for "significant impacts" from sites with "high" to "extreme" potential for shallow landsliding. If no such evidence or guidance exists, then I cannot concluide that CDF will have the ability to make the assessments that it will be required to do under this approach by means other than wishful thinking.

5. The habitat condition goals (page 60 and table 16 on page 61) are inadequate on both the technical and policy levels.

First, the habitat condition goals do "not constitute enforcement standards", and under this plan no aspect of habitat condition is in the end enforceable. Leaving aside for a minute the issue of whether the habitat goals themselves are appropriate, it is unreasonable to expect that unenforceable standards will effectively guide both site-specific decisions and the larger-scale plans within which such decisions will be made.

ORM-11

Even if the habitat goals were enforceable it is hard to see how they would not be met, given that the pool frequency goal, for example, reflects the minimum that would be expected in any case for the pool-riffle channels that typically compose coho habitat (Montgomery et al., in press). This is a very important issue because if habitat goals are set so that even poor-quality habitat is defined as properly functioning, then poor habitat has in effect become the goal. Specifically, for channels with gradients less than 3 to 4%, a rough upper limit to channels typically used by anadromous salmon, the target pool spacing is "1 pool every 6 bankfull channel widths" of channel length. While I applaud casting the pool frequency standard in terms of channel width, I am perplexed as to why one would pick a standard (6 channel widths) that is equal to what one would expect to see even in a pool-riffle channel (Leopold et al., 1964; Keller and Melhorn, 1978) with poor-quality habitat and very low wood loading (Montgomery et al., 1995). Moreover, the published data on pool frequency vs. wood loading (Montgomery et al., 1995) show that some forest channels that are even severely depleted in wood debris still meet the proposed goal. Also a goal for a minimum pool area of 20% of the channel surface area should not be hard to meet even in a severely degraded channel. In my opinion, the proposed standards simply codify poor to mediocre habitat as the goal for channel condition. A condition that the system should meet, whether in poor or good shape, does not meet the criteria for an interpretation of "properly functioning condition".

ORM-12

A single standard for pool frequency makes little sense as a blanket prescription for mountain channel networks because different types of channels have different ranges of pool frequency both with and without LWD. As mentioned above, the pool-riffle channels that compose the majority of the <1.5% gradient channels that compose coho habitat typically have a pool frequency of 5 to 7 channel widths

even when no wood is present. Addition of wood debris systematically reduces the pool frequency (Montgomery et al., 1995), but most channels with even little to no LWD will meet a pool frequency standard of 6 channel widths. The one channel type for which the proposed standard makes some sense is the "plane-bed" channels which typically occur on gradients of roughly 1.5 to 3% (Montgomery and Buffington, 1997). In these channels the pool frequency at low wood loading could be well greater than 6 channel widths due to the presence of only a few widely-spaced pools in the absence of mechanisms to force pool formation, such as wood debris. The available research for even plane-bed channels, however, shows that pool frequencies of 2 to 4 channel widths are more typical for moderate to high wood loadings that characterize old-growth channels (Montgomery et al., 1995). For the step-pool channels that typically characterize streams that are steeper than about 3 to 4%, typical pool spacings are 1 to 4 channel widths independent of wood loading. Hence, they should meet the standard whether or not there is any wood in the streams; it should prove almost impossible to fail to meet a standard of 1 pool per 6 channel widths in these channels.

DRM-12 con.

Another inadequacy in the "habitat elements" section of the "Properly functioning condition" matrix is that the size of woody debris plays no role in assessing "properly functioning condition", even though it is known that wood debris size can significantly affect its functionality in streams (e.g., Montgomery et al., 1996).

6. The attempt to identify the "channel migration zone" (CMZ) by somehow forcasting where on the floodplain the channel may wander over the 50 year life of the plan is a misguided attempt to implement a good idea. The plan is to be applauded for recognizing the importance of including such a migration zone within stream buffers. However, the attempt to base identification of the CMZ on the projected location of the channel over the 50 year life of the plan is clearly based on political considerations rather than technical ones. Let me clarify the basis for this opinion. We can argue that the active floodplain defines a zone in which the channel may reasonably migrate over century to millenial time scales. We may even be tempted to use recent records of channel migration rates (determined, for example, from aerial photographs) to forcast future channel positions. However, in forest channels large-scale avulsions can move the channel across the floodplain in discrete steps rather than as a progressive, incremental process. In other words, past rates of gradual channel migration (at say the outside of meander

DRM-13

bends) may give a reasonable estimate of average rates of channel migration, but they do not necessarily provide a solid basis for predicting future channel locations. And yet this would be required to identify where the channel may be over the 50 year plan. It is far more defensible to argue that the channel may occupy any location on its active floodplain over the life of the plan (unless particular local conditions effectively preclude it). ORM-13 con.

7. There is no technical justification for the extremely narrow (10 m) no cut buffers proposed for the Class I streams. The basis for the proposed retention levels for the "Limited Entry" zone of the buffers is not made clear. Furthermore, if the channel does move over the course of the plan such narrow no cut buffers may end up nowhere near the stream and therefore provide none of the functions desired from a riparian buffer. Moreover, it is difficult to evaluate how the post-watershed analysis buffers may function in regard to meeting the objectives of the plan as the "appropriate" buffer width based on such assessments may range from 30' to 170', and no guidelines are presented to illuminate the priorities upon which the assessment of what is most appropriate will be based. Knowing whether the ultimate buffer width decisions will be made to minimize risk to fish or to minimize inconvenience to PALCO is crucial for evaluating the potential for success of the proposed approach, but I could find no relevant guidance in the HCP. ORM-14

8. The 10' no cut buffers proposed for Class II streams are likely to be ineffective, as there may or may not be any large trees within 10' of the channel. I have been in streams with 25' buffers that are totally ineffective due to a dearth of large trees within 25' of the channel. ORM-15

9. The retention standards given in Table 17 allow the removal of all trees that exceed 40" DBH in spite of the fact that it is these large trees that have been shown to be necessary to catalyze log jam formation and the associated habitat elements in moderate to large-size channels (e.g., Abbe and Montgomery, 1996; Montgomery et al., 1996). There is no technical justification for selectively removing the largest trees from the riparian zone. Under both the Class I and Class II standards, only 12% or less of the total "residual basal area" will come from trees greater than 1 m DBH - even though studies have shown that it is trees 1 m in diameter or larger that provide stable in-channel wood in larger fish-bearing streams (e.g., Abbe and Montgomery, 1996; Montgomery et al., 1996). ORM-16

10. The equipment limitation zones proposed for Class III streams are not stream buffers in the sense of leaving trees along streams. While equipment limitation zones will reduce direct impacts to channels, the approach will not address the indirect effects of channel-proximal logging on these streams (e.g., shade, LWD recruitment, and the like). DRM-17

11. In conjuction with the material in the "Measures as Applied to List A Fish Species" on pages 70 and 71, there is no analysis of whether the reductions in sediment loading and increase in LWD recruitment in respect to *current levels* will be sufficient to *reverse* the trend in fish population declines. It is not enough to argue that by reducing the impact that the trends will change; what must be shown is that the effect will do more than simply decrease the rate of species decline. DRM-18

Volume II.
Part D: Landscape Assessment of Geomorphic Sensitivity

Background Section

The following statement on page 1 of part D is not strictly true: "limitations in the state-of-the-art, as well as data limitations for most wildland watersheds, precludes development of a quantitative, process-based model to predict absolute watershed sensitivity." The problem with this statement is several fold. First "sensitivity" cannot be absolutely known in any case since it reflects the POTENTIAL for response rather than a characterization of an actual response. Second, there are quantitative, process-based models available for gaging aspects of watershed sensitivity to landsliding and erosion problems. While none of the available (or even possible) approaches can predict with "absolute" certainty what will happen in a watershed in response to a praticular managment action, it is quite feasible to quantitatively predict how the sensitivity to particular processes or actions varies across watersheds, even where there is a paucity of information. I agree, however, with the idea that field assessments must be part of the effort to gage sensitivity. DRM-19

Approach Section

According to my interpretation of the landslide risk matrix on pages 3 and 4, slopes with the potential for generating debris flows (i.e., steep finely dissected slopes with no evidence of current "activity") could be classified as "high", "very high", or "extreme" risk of shallow DRM-20

landsliding. But based on the material presented in Volume 1, only on the "extreme" category would clear cutting not be conducted. Instead on the high and very high risk slopes, "alternative" methods would be examined. Elsewhere the plan states that "hollows" will be considered as extreme risk. Hence, the plan is not clear about how potential debris flow source areas will be treated.

DRM-20 con.

Part E: Assessment of Watershed Disturbances and Recovery

I concur that the disturbance index values calculated in this section "have no concrete meaning" (page 2). The location and juxtaposition of past actions and present states across a landscape has a huge influence on the net "disturbance" that it imparts on aquatic ecosystems. For example, if the clear cuts summed into Table 2 are all in the steepest most slide prone portion of the watershed, then their aggregated "disturbance" index should be much greater than if they were widely distributed among relatively stable portions of the catchment. The single "add 'em up" approach to such an index is an inadequate basis for drawing the desired understanding of landscape state out of such data.

DRM-21

Part F: Stream Monitoring Report

Just doing a pebble count is inadequate for determining "whether the stream is sediment loaded" (page 1).

DRM-22

The section on "Fine Sediments" contains a number of bizarre thoughts. First, the comment that the (undocumented) "high variability within streams would allow a discriminating salmonid to find good quality gravels in almost any stream in almost any year" is disingenuous (at best). Little is known about how far salmon will search for the "right" conditions and gravel for spawning, and even a discriminating salmon (in the sense employed by the document writer) may not be able to predict where loggin gactivities will increase fine sediment loading after it spawns. The statement about "discriminating salmon" can only be viewed as an odd mix of hubris and hope.

The measure of "pools per mile" is not meaningful without reference to the size of the channel. "Channel widths per pool" is a better metric for evaluating pool frequency.

Part H: Fisheries and Watershed Assessment

There are a number of problems with this section, among them:

"Rosgen channel type" is listed as an "indicator of geomorphic condition" in Table 1. It is no such thing. Rosgen's (1994) types are defined primarily by slope and grain size classes (C1, B2, etc...) and are primarly driven by channel slope (which sets the main alphabetic headings). Yet, channel slope is not a very good indicator of channel condition. Slope (and to some degree Rosgen's types) may be a reasonable indicator of the potential importance of some types of channel response, but Rosgen types say nothing interpretive about condition (although they do include some simple descriptive attributes like bed material type). Similarly, the discussion about D50 and stream bed sediment confuses discussion of bedload and suspended load (coarse and fine sediment) and glosses over the distinction between surface and sub-surface grain size. Yet, an understanding of how sediment moves through channels and how to read the evidence for these processes is central to the ability to decipher the influence of land management on stream conditions. Furthermore, the discussion argues that the goal should be coarser streambeds everywhere, regardless of the importance of geomorphic context in coming to such a conclusion. In my assessment, the inclusion of such fundamental misconceptions in this document undermines the credibility of the plan.

DRM-23

In this section yet another system for assessing potential for shallow mass wasting is presented, but here the assessment relies on slopes greater than 65 - 70% as being at high risk. [as a technical aside, one cannot use a range of values to set a lower limit to something that in order to be used inherently must have a single discrete value] Based on the variety of mutually inconsistent approaches to assessing potential mass wasting hazards presented in the HCP, I don't see how NMFS could conclude that it could come to the conclusion that mass wasting processes will be adequately addressed through future work because the basis for those assessments is not adequately constrained.

DRM-24

The demonstration in section 1.4.2 that temperature, and percent fine sediment do not correlate with "PL's calculated Disturbance Index" can be fairly interpreted to mean that the disturbance index is hokey. The alternative interpretation is, of course, that there is no detectable management influence on temperature or fine sediment loading. There are sound reasons to support the former interpretation, and yet PALCO's consultants conclude that the latter interpretation is correct without either offering supporting evidence or identifying the logic upon which they based their opinion. In my

DRM-25

professional opinion, the former possibility is much more probable. Furthermore, a problem with disturbance indices in general (as also discussed in Part E) is that one could harvest only a small portion of a catchment, and therefore remain below a critical value of a disturbance index, but destroy the stream habitat by cutting in the wrong or most sensitive places. Disturbance indices only address the importance what one does on a landscape, but fail to incorporate the equally important issue of where one does it. Yet in mountain drainage basins this issue is vital: clear cutting on a flat slope carries no risk of landsliding, whereas that same action on a very steep, soil-mantled slope can be very risky. Hence, the potential impact from harvesting 10% of a catchment, for example, depends very much on what portion of the landscape that area represents. ORM-25

Section 1.4.3
The Humboldt Bay WAA is said to contain "healthy fish populations", but no reference is given to their status relative to historic levels. ORM-26

Table 3 (page 26).
In table 3, the watersheds listed as having high risk in one or more categories are downgraded to "moderate" risk in the overall risk assessment (last column of table). Hence, there is an implicit averaging of risks that is used to come to this conclusion, even though risks of this nature are not simply "averagable". In order to make a conservative assessment the highest risk should define the overall risk, and hence a high risk in any category would dictate an overall high risk. This would be a much more defensible conceptual model for handling such risk under the criteria for evaluating HCPs. ORM-27

Van Duzen WAA (page 31)
The report states that "the presence of cumulative impacts from management were not evident". This is not surprising given that the data collected previously are evaluated against metrics that are calibrated to relatively poor conditions anyway (e.g., the 6 channel widths per pool and >20% pools metrics)? ORM-28

Volume IV
Habitat Conservation Plans
Part D. Section 1: Aquatic Species Conservation Plan

A very serious ommission from the HCP is the lack of an analysis of what fish runs were like historically, how they have changed, and what the primary (or suspected primary) causes for those changes ORM-29

were. This is very fundamental background information against which to evaluate the scope and relevance of the proposed measures. Even a simple plot of number of fish through time would be helpful in this regard. DRM-29 con.

In writing comments on this section I will try not to repeat comments addressed above.

Table 1 lists Monitoring Studies as a mitigation measure for the activity of "grazing". I fail to see how the expected benefit of identifying "damage to the riparian area from grazing" will mitigate such damage. Simply knowing that damage has happened does nothing to mitigate that damage. Monitoring studies in and of themselves cannot constitute mitigation measures. DRM-30

Watershed Analysis also is inappropriately listed as a mitigation measure. Watershed analysis can enable identification of appropriate mitigation measures, but its effectiveness at addressing problems depends upon how the specific framework for implementing it is developed, and how the information collected in the analyses is actually put to use.

On page 4 it is noted that "80-92 percent of all sediment delivered to streams were from non-road sources" on PL lands. This material came from "in-unit" failures, which the HCP will not address with the proposed landslide mitigation measures because it has *a priori* determined that loss of root strength after harvesting does not significantly accelerate landsliding, a position at odds with current understanding (e.g., Sidle et al., 1985). DRM-31

On page 25, PL contends that "many of these measures have already been implemented by PL, and in some cases have been a component of PL's land management for years". If this is the case, then the current conditions in these watersheds should support healthy or stable coho populations if PL is to contend (on the next page) that the measures proposed "are expected to provide a high level of protection for aquatic resources on PL's lands". This issue should be evaluated in the HCP, rather than simply asserted. DRM-32

The agencies should be very careful about allowing the process of watershed analysis to result in highly modified prescriptions. The success or failure of the approach to watershed analysis outlined by the Washington State methodology lies in the process used to

translate the analyses into prescriptions (see also Montgomery et al., 1995). A recent critique of the Washington program found the prescription writing processes did not reflect the analyses conducted in the watershed analyses (Collins and Pess, 1997). Hence, the uncritical assumption that watershed anlaysis will address adequately the site-specific tailoring of land use prescriptions to the landscape remains a rather large and unvalidated assumption.

DRM-32 con.

On page 27 PALCO states that they will conduct sediment budgets as part of the watershed anlaysis process. Elsewhere they state that they intend to follow the Washington watershed anlaysis process. However, sediment budgets are not part of Washington's process. Such inconsistencies further compromise the credibility of the proposed HCP.

DRM-33

Section 1.2.1.1
Assessment of Road and Associated Sediment Sources
Conducting 4 road surveys every 10 years (or one every 2.5 years) is very slow. The proposed plan apparently allows cutting and road building in Larabee Creek, Salmon Creek, and the Matole and Bear Rivers in the first two decades of the plan before road surveys are conducted in these watersheds. This will likely result in adverse impacts to coho and coho habitat through failure to address road-related contributions to downstream sediment loading in the interim.

DRM-34

Section 1.2.2.1
Channel Migration Zone
Identification of the channel migration zone depends on a "qualified fluvial geomorphologist", but how one determines such qualifications is unclear and should be clarified in the HCP. This is an important question, as there is no degree program in "fluvial geomorphology" anywhere that I know of -- at the M.S. and Ph.D. level one can be trained in fluvial geomorphology, but at the schools that I am aware of one's degree says "geology, geology & geophysics, geological sciences" or something to that effect. There are many people now calling themselves "fluvial geomorphologists" who have no business doing so. The HCP should spell out how to tell a "qualified" person from a recent convert from another subdiscipline, or a different discipline (such as a civil engineer).

DRM-35

Section 1.2.2.5
Hillslope Management

The hillslope management section states that "for portions of the ownership lacking geology and soils maps necessary to make a determination of risk, PL is responsible for providing site specific risk ratings...". And yet, earlier PL states that they will not harvest in headwall swales (a.k.a. hillslope hollows) as they are part of the "extreme" hazard class. But even in areas that do have geology and soils maps the landslide risk cannot be ascertained from such maps because hillslope hollows cannot be seen on such maps. This is cause for concern because if it is the "extreme" risk maps (such as in Volume V) that trigger the "no harvest without geologist review" default prescription, but the method used to identify such "extreme" risk sites is incapable of identifying hollows, then the plan cannot follow through on the committment to avoid harvest in hollows. Even if hollows will be independently identified during the THP application process, I know of no demonstrated alternative silvicultural methods that could be employed on even "high" risk (as opposed to "extreme" risk) sites without significantly increasing the risk of shallow landsliding and catastrophic downstream impacts on aquatic ecosystems. ORM-36

Also, I do not know why the certification of Professional Registered Geologist is considered adequate for reviewing the potential slope for instability. As I understand it, the registration test for this certification has little to do with problems of applied hillslope geomorphology or the influence of vegetation on slope stability in forested terrain.

Section 1.2.7
Scientific Surveys and Monitoring
The HCP states that "PL already has a significant trends monitoring program in place on its lands". The data from such efforts should be analyzed and used to document past or current trends and the information should be included in the HCP. ORM-37

Section 1.2.9
Watershed Analysis
The caveats placed on the watershed analysis process proposed under the PL HCP effectively destroy the ability of such a process to fulfill its intended purpose in the manner discussed by Montgomery et al. (1995). In particular, the identified maximum limits precludes implementation of more stringent prescriptions in locations where critical habitat or other special conditions warrant extra care. ORM-38

A very serious shortcoming of the HCP is that the entire intent of the process of watershed analysis, and the rationale for its use to implement adaptive management and ecosystem managment efforts lies in the very things that PALCO reserves the right not to do on the bottom of page 47. In the second to last paragraph on the page, PALCO reserves the right to continue practices that are shown at a future date to be damaging if such practices fall under the pre-defined maximum protection. This is not adaptive managment. This is not using the best available science to drive decisions. This is not the basis upon which to grant a blank check for incidental take for 50 years, as it would mean that knowledge obtained during the life of the plan may not be used to compell changes to the plan -- even if such changes are discovered to be vital to achieving the plans objectives.

DRM-38 CON.

As I recall, I wrote the phrase of the Washington manual quoted on page 48 ("Periodic revision and incorporation of new methods and insight is a fundamental assumption of the diagnostic approach upon which this manual relies") as part of the original draft of the Channel Assessment Module, and I disagree that it has been lived up to in Washington. The methods have not changed much - not because they are perfect, but rather because there is no real process to revise and update the manual. Such revision is essential and it is untenable to defend the position of reserving the right not to update methods as new understanding emerges from monitoring a system.

The Pyles et al. (1998) report referred to as indicating that aerial photograph analyses "can significantly underestimate natural rates of mass wasting" is highly speculative and the conclusion they reach is likely flawed by failure to recognize that aerial photograph analysis also underestimates rates of post-harvest sliding in areas where revegetation by understory occurs quickly (like the Pacific Northwest). Moreover, in order for their conclusion to be true, then there must be hiding under a forest canopy enough landslides to account for the four to ten fold increase typically reported by aerial photograph inventories. There simply is no body of credible (i.e., peer reviewed and published) evidence to suggest that this is true.

DRM-39

Section 1.3.1 Overview

This section states that monitoring and adaptive management will be used to "make sure that the conservation strategy is effective in providing protection for aquatic resources", but enough limitations

DRM-40

and caveats are placed on what the agencies may require in so far as revised prescriptions are concerned that the statement rings hollow. DRM-40 con.

1.3.1.4 Channel Stability

The statement that the proposed HCP measures for RMZ's "can provide for LWD recruitment levels comparable to those in unharvested systems" contrasts with previous statements that it would be 50 to 70% of what is found in unharvested systems. A 30 to 50% difference is hardly "comparable". DRM-41

The interpretation of the management implications of Culp's (1988) work offered on page 70 is likely incorrect. Culp's work could also be interpreted to indicate how variable streambank erosion processes are and to illustrate the paramount importance of *in-channel* LWD for maintenance of channel bed elevations in some steep gravel-bed forest channels. The interpretation that even partial retention of riparian vegetation is "sufficient to prevent channel instability" is a classic example of inverse reasoning. The study appears to have demonstrated that full riparian harvest and LWD removal is sufficient to cause channel instability (in the sense of the term employed by the authors), but the lack of response at a particular place does not adequately test the general proposition that leaving non-merchantable timber is adequate to prevent channel instability. Moreover, the implication that harvesting of the largest trees in riparian zones will not impact channel morphology or "stability" runs counter to other recent studies that have demonstrated otherwise (e.g., Abbe and Montgomery, 1996; Montgomery et al., 1995; 1996).

On page 71, the authors of the aquatic species conservation plan (Vol. IV part D) state that "decreases in root strength following harvest, and the interval until regrowth could provide new sources of LWD, are probably much lower that (sic) in the reviewed studies". This statement is offered as part of an argument for why partial cutting beyond the narrow 30' no cut buffers will not impact channel stability. I am not aware of any data that substantiates this opinion and the basis for it appears to be the simple desire to ignore previous studies which show the importance of root strength reduction following harvest. It is well known that the finest roots die fast after harvest and it is reasonable to assume that upon removal of the canopy this occurs even in redwoods. The resprouting may lead to more rapid regrowth of the fine-scale root network in redwoods than one would find in, say, Douglas-fir forest, but there will still be a period of time in which the aggregate root DRM-42

strength in the soil will be dramatically reduced. I know of no evidence to suggest that this will be significantly shorter than the acknowledged 3 to 15 years for Douglas fir. The belief that resprouting of redwoods means that one doesn't have to consider the potential effects of post-harvest root strength reduction is simply an unsubstantiated belief. Furthermore, the Sustained Yield Plan documents that PALCO plans to convert much of the landscape to Douglas fir. If they are successful at such conversion then the root strength issue will become increasingly important in any case.

DRM-42 con.

1.3.1.6 Sediment Input-Mass Wasting

The statement that "quantifying the increase in landsliding due to management is informative, but has limited utility in the design of a HCP" (page 78) is bizarre. If the goal of an HCP is to reduce sediment loads to near background levels, then the rates of sliding due to management and in particular to so-called "in unit" slides is vitally important. Without such information, it is difficult to conclude that anything other than the no cut option on any *slide prone* terrain (i.e., the high, very high and extreme risk categories) will be adequate over the life-time of the plan for meeting the stated objectives of the plan. In this context, the lack of any credible comparision of contemporary rates of sliding to background rates is astounding. While it can be tricky to determine a background rate, it is not really all that difficult to constrain an estimate of the possible range of background rates, and such a study could probably be done for a fraction of the cost of the work that went into preparing this HCP.

DRM-43

The mass wasting section concludes that mass wasting is far more important than surface erosion from roads as a component of the sediment budget for creeks like Bear Creek in the area covered by the HCP. This conclusion is consistent with the general understanding that this part of Northern California is naturally prime landslide country. I am concerned about discrepencies in the manner in which the PWA reports are used in the HCP and in a report prepared for NMFS by Dr. Leslie Reid, an internationally recognized expert on sediment budgets. The problem I see is that the HCP draft states that based on the PWA study the number of "landslides under PL's current management regime is virtually unchanged from the number observed under old growth conditions" (page 79), giving the impression that current rates of sliding are not far from a background condition. In contrast, Dr. Reid's report, which used the same PWA report, concludes that "data from Bear Creek demonstrate that less intensive silvicultural practices than

clearcutting have increased the risk of hillslope failures resulting in sediment delivery to watercourses by 960%". These statements present rather different assessments of the impact of past and present management practices on sediment yields by landslide processes in Bear Creek. That a respected scientist of Dr. Reid's stature would come to a conclusion so at odds with the way in which the PWA report is cast in the HCP casts serious doubt on the credibility of the interpretation offered in the HCP.

ORM-43 con.

I was also astounded to read (on page 80) that the first conclusion that the HCP writers wring out of the PWA reports is that "natural landslides can constitute the majority of all sediment delivered to streams". Perhaps the HCP writers are relying on information not conveyed on the previous pages of the draft HCP to come to this conclusion, but there appears to be nothing presented in the HCP that supports this notion. Actually, on page 79 it is noted that only "20.1 percent of all landslides occurred in unmanaged areas"; by subtraction we can estimate that the vast majority, or about 80% of the landslides occurred in "managed" areas. The statements on page 79 also acknowledged that no estimate of "background" rates was in fact made. Therefore I do not understand how PALCO can support the conclusion that "natural" slides dominate the sediment delivered to streams. The fact that landslides are an important natural process in this region is not sufficient to make their point. In fact, it is far more reasonable to argue, based on our understanding of landscape evolution processes, that in a place that has evolved slopes steep enough to be naturally close to the threshold for slope instability, then significant changes in a process or factor that is a first-order influence on slope stability, such as in the effective cohesion provided by roots, should lead to a substantial increase in both the frequency and extent of landsliding.

The prescriptions for addressing the mass wasting hazard outlined on page 80 of volume IV section D are not likely to meet the goals of the HCP, even though some of the proposed measures are reasonable steps to take. For example, the plan proposes that a review by a geologist be conducted prior to road building or harvesting in areas marked as "extreme" hazard slopes, headwall swales, inner gorges or currently unstable areas, which together account for "65 percent of all mass wasting sites". This is not a bad idea, but there are several potential problems. If such a review is intended to ascertain whether or not these areas are actually like they are portrayed on planning documents or maps (or digital terrain models), then this is a

fine idea. There is no sense in restricting harvesting activities because of slope stability concerns if such concerns are based on incorrect portrayal of the landscape. But it is the stated intent of this plan that harvest of these marginally stable areas will proceed based on the opinions of geologists working for PALCO. I know of no study that demonstrates that silvicultural alternatives to clear cutting do not result in a significant increase in the potential for slope instability from these very sensitive portions of a landscape. Any prescriptions that result in partial or shelterwood cutting in these areas amount to uncontrolled experiments in hazardous terrain. That is not what HCPs are designed to do. Furthermore, no such review is required before cutting timber in "high" or "very high" mass wasting hazard terrain. Consulting Selby's (1993) "Hillslope Materials and Processes" or Sidle et al.'s (1985) "Hillslope Stability and Land Use" will lead one to conclude that timber harvets on slopes with a high to very high landslide hazard will most likely increase rates of landsliding.

DRM-43 con.

Prescriptions proposed for recent THP's suggest that there is ample cause for concern along these lines. The following comments are also based on my reading of Dr. Reid's report. It appears that in prescriptions developed for Sulphur Creek, PALCO essentially ignored the geologists recommendations to "avoid the most sensitive areas of the hillside" which should also include "headwall swales" by the definition in the draft HCP. According to Dr. Reid, 18 of 60 acres of "headwall swales" were to be clearcut with the rest selectivley logged. It is astounding that this was decided not to present a significant risk of accelerated landsliding. I know of no independent study that would support this recommendation on a mechanistic basis. If the ultimate prescriptions employed in implementing the HCP are to run counter to our general understanding of the controls on slope stability in steep forested terrain, then it is a sham to bother with such a review in the first place.

2.3 Adaptive Management / 2.4 Additional Modifications to the Aquatic Strategy

DRM-44

The approach to adaptive management specified in the draft HCP violates the definition offered at the start of the section that discusses it. Based on my reading of Sections 2.3 and 2.4, I am forced to conclude that either those drafting the plan do not understand the basic concept of adaptive managment or they are being disingenuous in calling their proposal adaptive management.

ORM-45

The definition offered of adaptive management is standard: "the first essential characterisitic of adaptive management is that a direct feedback loop exists between science and management ... the second essential characterisitc of adaptive management is that management is an experiment". A key shortcoming of the plan is the "sideboards" placed on the process which preclude confidently concluding that the HCP will result in a process that exhibits these 2 essential characteristics of adaptive management.

First, there is no "direct feedback loop between science and mangement" in the plan. The only potential trigger for changing management based on monitoring results is if the *predetermined* aspects of the system that are being monitored from initial implementation of the plan change in an undesireable manner. Our understanding of forest and aquatic ecosystems will evolve over the life of the plan, but according to my reading of this document new understanding will not be allowable as the basis for altering management decisions. Even in the case where monitoring results lead to the identification of a particular problem, the process outlined will lead to "consultations" with an unclear abilty of the agencies to mandate updated practices adequate to solve any such problems.

A rather fundamental problem with the proposed "modifications to the aquatic strategy" contained in section 2.4 is that it tosses out the goal of tailoring landuse to the landscape by making PALCO subject to the minimum standards for coho prescriptions imposed on *any* "private timber owner in the Southern Oregon/Northern California Coast ESU" (page 113). A fundamental concept behind the idea of watershed analysis is that different landscapes (and hence different ownerships) possess different inherent sensitivities to land use. For implementation of the ESA, for example, watershed analyses may result in different standards for streams in different locations or ownerships due to either differences in geology, the legacy from past land use, or the condition of local fish runs. In my opinion, the modifications outlined in section 2.4 undermine the conceptual basis for the entire HCP in that they guarantee that the prescriptions in the HCP will be the most restrictive that could be imposed on PALCO *no matter what we learn about the landscape, stream ecology, or the status of coho salmon in the plan area*, while at the same time it mandates that PALCO's activities will be no more restricted than anyone else's in the region *no matter what the differences in the landscape or habitat processes or condition and biological status*

between ownerships. Put simply, this is not adaptive management, and it should not be advertised as such.

ORM-45 con.

Volume V
Maps

1. For a region that is widely acknowledged to be extremely prone to landsliding, and in which PALCO makes a point of how naturally unstable the terrain is, it is amazing that about 1% of the area is shown as having an "extreme" landslide hazard index.

ORM-46

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